# इंटरनेट मानक

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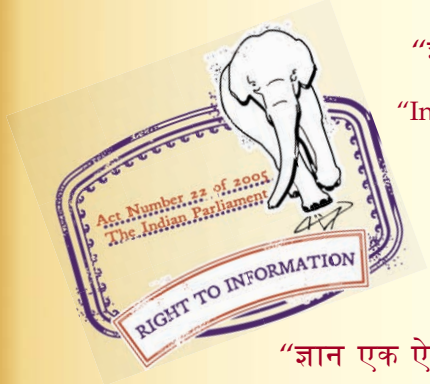
IS 7920-3 (1996): Statistical vocabulary and symbols, Part 3: Design of experiments [MSD 3: Statistical Methods for Quality and Reliability]

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"ज्ञान एक ऐसा खजाना है जो कभी चुराया नहीं जा सकता है"
Bhartṛhari—Nītiśatakam
"Knowledge is such a treasure which cannot be stolen"

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( Reaffirmed 2002 )

भारतीय मानक

# सांख्यिकीय शब्दावली और प्रतीक

## भाग 3 प्रयोगों का अभिकल्प

*( दूसरा पुनरीक्षण )*

*Indian Standard*

# STATISTICAL VOCABULARY AND SYMBOLS

## PART 3 DESIGN OF EXPERIMENTS

*( Second Revision )*

ICS 01. 040. 03; 01.080. 03; 03.120. 30



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## FOREWORD

This Indian Standard (Second Revision) was adopted by the Bureau of Indian Standards, after the draft finalized by the Statistical Methods for Quality and Reliability Sectional Committee had been approved by the Management and Systems Division Council.

The Indian Standard covering the terminology of statistical terms used in quality control and sampling, IS 7920, was first published in 1976 and revised in 1985. Since the publication of the revised standard in 1985, many other standards have been published in this field using additional terms. It was also felt that the definitions of some of the terms covered in IS 7920 : 1985 'Statistical vocabulary' need modification. At the international level, the Technical Committee on Application of Statistical Methods (ISO/TC 69), has also published International Standards on statistical vocabulary as the three parts of ISO 3534. Keeping in view the above, this revision of the vocabulary on statistical terms has been taken up.

In line with the three parts of ISO 3534, the terminology has been divided in the following three parts of IS 7920:

Part 1 Probability and general statistical terms

Part 2 Statistical quality control

Part 3 Design of experiments

In preparing this part, considerable assistance has been derived from the definitions given in ISO 3534-3 : 1989 'Statistics — Vocabulary and symbols Part 3: Design of experiments'.

The examples accompanying the definitions of certain terms are generally intended to illustrate simple applications of those definitions and to provide the experienced person with a reference to illustrate the concepts to less experienced practitioners.

Background information on the design of experiments is given in Annex A.

An informative list of published Indian Standards on Statistical Methods is given at Annex B.

For ease of reference, an alphabetical index to the terminology has been provided at Annex D.

The composition of the Committee responsible for the formulation of this standard is given at Annex C.

# *Indian Standard*

# STATISTICAL VOCABULARY AND SYMBOLS

## PART 3 DESIGN OF EXPERIMENTS

## *( Second Revision )*

### 1 SCOPE

This standard (Part 3) gives definitions of the terms used in the field of design of experiments. The terms are classified into the following logical groups:

a) General terms,
b) Types of designs, and
c) Methods of analysis.

### 2 GENERAL TERMS

#### 2.1 Design of Experiment; Experiment Design

A planned arrangement in which an experimental programme is to be conducted, and the selection of the levels (versions) of one or more factors or factor combinations to be included in the experiment.

NOTE — The purpose of designing an experiment is to provide the most efficient and economical methods of reaching valid and relevant conclusions from the experiment. The selection of an appropriate design for any experiment is a function of many considerations such as the type of questions to be answered, the degree of generality to be attached to the conclusions, the magnitude of the effect for which a high probability of detection (power) is desired, the homogeneity of the experimental units and the cost of performing the experiment. A properly designed experiment will permit relatively simple statistical interpretation of the results, which may not be possible otherwise. The 'arrangement' includes the randomization procedure for allocating treatments to experimental units.

#### 2.2 Factor

An assignable cause which may affect the responses (test results) and of which different levels (versions) are included in the experiment.

NOTE — Factors may be quantitative, such as temperature, speed of execution and voltage applied, or they may be qualitative, such as the variety of a material, presence or absence of a catalyst, and the type of equipment.

Those factors which are to be studied in the experiment are sometimes called 'principal factors'.

#### 2.3 Level; Version (of a Factor)

A given value, a specification of procedure or a specific setting of a factor.

*Example:*

Two versions of a catalyst may be presence and absence. Four levels of a heat treatment may be 100°C, 120°C, 140°C and 160°C.

NOTE — 'Version' is a general term applied both to quantitative and qualitative factors. The more restrictive term 'level' is frequently used to express more precisely the quantitative characteristic.

Responses observed at the various levels of a factor provide information for determining the effect of the factor within the range of levels of the experiment. Extrapolation beyond the range of these levels is usually inappropriate without a firm basis for assuming model relationships. Interpolation within the range may depend on the number of levels and the spacing of these levels. It is usually reasonable to interpolate, although it is possible to have discontinuous or multimodal relationships that cause abrupt changes within the range of the experiment. The levels may be limited to certain selected fixed values (known or unknown) or they may represent purely random selection over the range to be studied. The method of analysis is dependent on this selection.

#### 2.4 Treatment

Levels or combination of levels (versions) of each of the factors assigned to an experimental unit (*see* 2.5).

#### 2.5 Experimental Unit

Entity to which a treatment is applied or assigned in the experiment.

*Example*:

The unit may be a patient in a hospital, a group of animals, a production batch, a section of a compartmented tray, a plot of land, etc.

#### 2.6 Experimental Error

It is that part of the total variability in the responses (test results) which is not due to any assignable cause like, factors and blocks, or which is not

associated with any deliberate variation in the experimental conditions and which introduces a degree of uncertainty in the conclusions that are drawn from the experimental results.

NOTES

1 It is a common characteristic of experiments that, when they are repeated, their results vary from trial to trial, even though the experimental materials, environmental conditions and the experimental operations are carefully controlled. Therefore, the occurrence of experimental error is inevitable in practical experimentation. This variation introduces a degree of uncertainty into conclusions that are drawn from the results, and therefore has to be taken into account in reaching conclusions. Experimental error is usually measured in an experiment as a pooled variance of sets of duplicate observations for the same treatment.

2 Experimental error is the unexplained or residual part of the total variation.

### 2.7 Block

A group of relatively homogenous experimental units.

NOTE — Blocks are usually selected to allow for assignable causes, in addition to those introduced as principal factors to be studied which it may be difficult, or even impossible, to keep constant for all of the experimental units in the complete experiment. The effect of these assignable causes may be minimized within blocks. The analysis of the experiment results has to take into account the effects of blocks.

Blocks which accommodate a complete set of treatment combinations are called 'complete blocks'. Those which accommodate only a portion of the complete set are called 'incomplete blocks'. In a paired comparison experiment, where two treatments are dealtwith in pairs, the pairs are considered as 'blocks'.

*Example*

The term 'block' originated in agricultural experiments in which a field was subdivided into sections having common conditions, such as exposure to the wind, proximity to underground water or thickness of the arable layer. In other situations, blocks are based on batches of raw material, operators, the number of units studied in a day, etc.

### 2.8 Block Factors

Those assignable causes which form the basis for grouping the experimental units into blocks.

NOTE — Generally the versions of the block factors are imposed by the available experimental conditions, but sometimes they are selected in order to broaden the interpretation of the results by including a wider range of conditions. It is usually assumed that the block factors do not interact with the principal factors. When the versions are relatively close, this hypothesis is often a reasonable assumption. However, if the versions differ considerably or if there is no priority basis for the assumption, the assumption should be verified so that an appropriate method of analysis may be chosen for the experimental data.

### 2.9 Replication

The repetition of a complete set of all the treatments to be compared in an experiment. Each of the repetitions is called a 'replicate'.

NOTE — Since experimental error is almost invariably present, replication is required to increase the precision of its estimate. In order to do this effectively, all elements contributing to the experimental error should be included in the replication process. For some experiments, replication may be limited to repetition under essentially the same conditions, such as the same facility or location, a short time interval or a common batch of materials.

For other experiments requiring more general results, replication may require deliberately different, though similar, conditions, such as different facilities or locations, longer time intervals or different batches of materials.

In some experiments, a 'pseudo-replication' occurs when factors which produce no effect (average or differential) are included in the experiment.

When a subset of the treatments within an experiment is repeated, this is generally referred to as 'partial replication'.

### 2.10 Duplication

The execution of a treatment more than once under similar conditions. For example, repetition of the same treatment to different experimental units in the same block.

NOTE — Duplication, as contrasted to replication, refers to a single element of an experiment. Duplication usually involves a fresh experimental unit, such as a new sample, or, when a single unit is involved, an independent resetting of the levels of the factors being studied on that unit. When duplicate observations are made on the same sample or unchanged settings, these should be identified as duplicate observations rather than as 'duplicates' to reflect the narrower degree of duplication. Recent usage has broadened the definition of duplication to more than once rather than restricting it to twice.

### 2.11 Randomization

The procedure used to allot treatments at random to the experimental units.

NOTES

1 An essential element in the design of experiment is to provide estimates of effects free from biases due to undetected assignable causes. Randomization is a process to minimize this risk. The operational procedure for assignment 'at random' involves the use of Random Numbers or some similar method for assuring that each unit has an equal chance of being selected for each treatment.

2 Randomization provides independence in the observations.

### 2.12 Main Effect; Average Effect

A term describing a measure for the comparison of the responses at each level (version) of a factor

averaged over all levels (versions) of other factors in the experiment.

NOTE — It should be noted that even though a 'main effect' is indicated to be small, this does not necessarily mean that the factor is not important. Large effects of the factor may result at various levels (versions) of other factors, but may differ in sign and/or magnitude.

The process of averaging in these cases would tend to make the 'main effect' appear smaller *see* **2.13**.

In a model, the term 'main effect' may describe the parameter or the estimate of this parameter *see* **2.21**.

### 2.13 Interaction

A term describing a measure of dependence of the level (version) of one factor on the level(s) of other factor(s) by providing the differential comparison of the responses for each level (version) of the factor on each of the several levels (versions) of other factor(s).

NOTE — When an interaction is determined to be of sufficient magnitude, it is implied that the effect of variation within the factor is dependent upon the levels (versions) of the other factors. Since an 'interaction' indicates a 'differential effect'. The effects of these factors should not simply be described in terms of averages over all levels (versions) of the other factors ('main effects') involved, but separately for each such level (version).

*Example*:

| | Factor *A* | | | | Average effect of *B* |
|---|---|---|---|---|---|
| Factor *B* | Version | 1 | 2 | 3 | |
| Version | 1 | 10 | 22 | 28 | 20 |
| | 2 | 20 | 20 | 20 | 20 |
| Average effect of *A* | | 15 | 21 | 24 | |

An interpretation of these results, assuming little experimental error, is that changes in version of factor *A* affect the responses when using version 1 of factor *B*, but do not affect the responses when using version 2. Also, changes in version of factor *B* affect the responses when using version 1 of factor *A* in an opposite direction to when version 3 of factor *A* is used, etc. Note that the response of factor *A* at version 2 of factor *B* would show no effect. If the results for version 2 of factor *B* had been 8, 20 and 26, there would be no interaction since the differences between the results of the two versions of *B* at each version of *A* (or the 3 versions of *A* at each version of *B*) would be constant.

An interaction involving two factors (*AB*) is called a 'two-factor interaction' one involving three factors (*ABC*) is called 'three-factor interaction', etc.

### 2.14 Confounding

Combining indistinguishably the main effect of a factor or a differential effect between factors (interactions) with the effects of other factor(s), block factors(s) or interaction(s).

NOTE — Confounding is an important technique which permits the effective use of specified blocks in some experiment designs. This is accomplished by deliberately preselecting certain effects or differential effects, as being of little interest, and planning the design so that they are confounded with block effects or other pre-selected principal factor or differential effects, while keeping the other more important effects free from such complications. Sometimes, however, confounding results from inadvertent changes to a design during the running of an experiment or from incomplete planning of the design, and it diminishes or invalidates the effectiveness of an experiment.

The confounded factorial design (**3.16**) and factorial experiment with partial confounding (**3.17**) are examples of confounding effects or differential effects (interactions) with block effects. See fractional factorial design (**3.19**) and aliases (**2.18**), each effect is confounded with one or more other effects.

### 2.15 Contrast

A linear function of the observations or parameters for which the sum of the coefficients is zero. With observations $Y_1, Y_2 \ldots Y_n$, the linear function $a_1Y_1 + a_2Y_2 + \ldots + a_nY_n$ is a contrast if and only if, $\Sigma a_i = 0$; not all $a_i$'s are equal to zero.

*Example* 1:

A factor is applied at three levels and the results are represented by $A_1$, $A_2$, $A_3$. If the levels are equally spaced, the first question it might be logical to ask is whether there is an overall linear trend. This could be done by comparing $A_1$ and $A_3$, the extremes of $A$ in the experiment. A second question might be whether there is evidence that the response pattern shows curvature rather than a simple linear trend. Here the average of $A_1$ and $A_3$ could be compared to $A_2$ (If there is no curvature, $A_2$ should fall on the connecting $A_1$ and $A_3$ or, in other words, be equal to their average).

| *Response* | $A_1$ | $A_2$ | $A_3$ |
|---|---|---|---|
| Contrast coefficients for question 1 | −1 | 0 | +1 |
| Contrast 1 | $-A_1$ | | $+A_3$ |
| Contrast coefficients for question 2 | −1/2 | +1 | −1/2 |
| Contrast 2 | $-1/2A_1$ | $+A_2$ | $-1/2A_3$ |

This example illustrates a regression type study of equally spaced continuous variables. It is frequently more convenient to use integers rather than fractions for contrast coefficients. In such a case,

the coefficients for contrast 2 would appear as (− 1, +2, − 1).

*Example* 2:

Another example dealing with discrete versions of a factor might lead to a different pair of questions. Let us suppose there are three sources of supply, one of which, $A_1$, uses a new manufacturing technique while the other two, $A_2$ and $A_3$, use the customary one. First, does vendor $A_1$ with the new technique seem to differ from $A_2$ and $A_3$ which are using the old one? Contrast $A_1$ with the average of $A_2$ and $A_3$. Second, do the two suppliers using the customary technique differ? Contrast $A_2$ and $A_3$. The pattern of contrast coefficients is similar to that for the previous problem, though the interpretation of the results will differ.

| *Response* | $A_1$ | $A_2$ | $A_3$ |
|---|---|---|---|
| Contrast coefficients for question 1 | −2 | + 1 | + 1 |
| Contrast 1 | $-2A_1$ | $+A_2$ | $+A_3$ |
| Contrast coefficients for question 2 | 0 | − 1 | + 1 |
| Contrast 2 | | $-A_2$ | $+A_3$ |

NOTE — The coefficients for a contrast may be selected arbitrarily provided that $\Sigma a_i = 0$ condition is met. Questions of logical interest from an experiment may be expressed as contrasts with carefully selected coefficients. See examples 1 and 2 above. As indicated in the examples, the response for each treatment combination will have a set of coefficients associated with it. Sometimes the term 'contrast' is used only to refer to the pattern of the coefficients, but the usual meaning of this term is the algebraic sum of the responses multiplied by the appropriate coefficients.

### 2.16 Orthogonal Contrasts

Two contrasts are orthogonal if the contrast coefficients of the two sets satisfy the condition that, when multiplied in corresponding pairs, the sum of those products is equal to zero.

*Example* 1:

| | | $A_1$ | $A_2$ | $A_3$ |
|---|---|---|---|---|
| $a_{i1}$ | Contrast 1 | −1 | 0 | + 1 |
| $a_{i2}$ | Contrast 2 | 0 | − 1 | +1 |
| $a_{i1}a_{i2}$ | | 0 | 0 | +1 |

$\Sigma\, a_{i1}\, a_{i2} = 1$ Therefore, not orthogonal

*Example* 2:

| | | $A_1$ | $A_2$ | $A_3$ |
|---|---|---|---|---|
| $a_{i1}$ | Contrast 1 | − 1 | 0 | + 1 |
| $a_{i2}$ | Contrast 2 | − 1 | + 2 | − 1 |
| $a_{i1}\, a_{i2}$ | | + 1 | 0 | −1 |

$\Sigma\, a_{i1}\, a_{i2} = 0$ Therefore, orthogonal

### 2.17 Orthogonal Array

An array of order $k \times N$ with entries from a set of $s$ symbols is an orthogonal array with $k$ constraints (or factors), $N$ assemblies, $s$ symbols and strength $t$, if in $t \times N$ sub-array of the $k \times N$ array, every ordered $t$-plet of $s$ symbols occurs equally often.

NOTE — The columns of an orthogonal array form the various factors and rows represent the ($N$) treatments.

### 2.18 Aliases

Two or more effects (main or interaction) in a fractional factorial experiment, a linear combination of which is estimable but they can not be estimated separately.

NOTES

1 In a $2^n$ fractional factorial design, the aliases can be determined once the 'defining contrast' (in the case of a half replicate) or 'defining contrasts' (for a fraction smaller than half) are stated. The defining contrast is that effect (or effects), usually thought to be of no consequence, about which all information may be sacrificed for the experiment. An identity I is equated to the defining contrast (or defining contrasts) and using the convention that $A^2 = B^2 = C^2 = I$, the multiplication of the letters on both sides of the equation shows the aliases. In the example of **3.19**

$$I = ABCD$$

so that, $A = A^2BCD = BCD$

and $AB = A^2B^2CD = CD$

Here $A$ and $BCD$ are aliases. If we assume that one of them is negligible, the other can be estimated.

2 With a large number of factors (and factorial treatment combinations) the size of the experiment can be reduced to 1/4, 1/8, or in general to $1/2^k$ to form a $2^{n-k}$ fractional factorial experiment.

3 There exist generalizations of the above to fractional factorials having more than 2 levels.

### 2.19 Predictor Variable

A variable the levels (versions) of which are selected, such as a factor level in an experiment, whether the selection is in the control of the experimenter or not.

NOTE — This is sometimes referred to as an 'independent variable'.

### 2.20 Response Variable

The variable that shows the observed results of an experimental treatment.

NOTE — This is sometimes referred to as the 'dependent variable'.

### 2.21 Model

An equation which is intended to provide a functional description of the observations/sources of information in terms of parameters. The form of the model can be expressed as:

Observed value = Σ(Parameters or terms representing assignable causes/factors)
+ Σ(Random variables representing assignable effects)
+ Random variable representing non-assignable effects (also called residual error)

NOTE — The assumptions about the residual error component are as under:

a) The expected value of each residual random variable is zero.

b) The residual random variables are mutually independent.

c) All the residual random variables have the same variance.

d) Each of residual random variable is normally distributed.

*Example*:

Two quantitative factors ($X_1$) at two levels and ($X_2$) at three levels, are to be studied in a 2×3 factorial experiment (*see* **3.12**) with replication in two blocks (a randomized block design). If it is assumed, as is frequently the case, that the response pattern can be approximated by a polynomial model, the fitted equation of the model can be written as:

$$Y = B_0 + B_1X_1 + B_2X_2 + B_{22}X_2^2 + B_{12}X_1X_2 + B_bX_b + e$$

where

$Y$ = observation corresponding to a treatment
$B_0$ = constant
$B_1$ = linear effect of factor $X_1$
$B_2$ = linear effect of factor $X_2$
$B_{22}$ = curvature effect of factor $X_2$
$B_{12}$ = Interaction (Differential) effect of the two factors (linear)
$B_b$ = effect of blocks, and
$e$ = random error

### 2.22 Response Surface

It is a functional relationship between the predictor variable and the response variable.

NOTE — A sequential form of experimentation is often used in conjunction with the mapping of response surfaces in which the responses of the earlier phases are used to help predict where to select additional treatment combinations for study so as to optimize results efficiently.

### 2.23 Evolutionary Operation (EVOP)

A sequential form of experimentation conducted in production facilities during regular production.

NOTE — The principal theses of EVOP are that knowledge to improve the process should be obtained along with a product, and that designed experiments using relatively small shifts in factor levels (within production tolerances) can yield this knowledge at minimum cost. Change of versions (levels) of the factors for any EVOP experiment is usually quite small in order to avoid making out-of-tolerance products, and this may require considerable replication so as to improve the efficiency of comparison of the effect of the change in levels.

## 3 TYPES OF DESIGNS

### 3.1 Completely Randomized Design

A design in which the treatments are assigned at random to the full set of experimental units.

NOTES

1 No block factors are involved in a completely randomized design.

2 Completely randomized design is advocated only under the assumption that all the experimental units are more or less homogenous.

### 3.2 Randomized Block Design

A design in which the experimental units are grouped into blocks, the units within each block being more homogenous than units in different blocks. Each block contains as many experimental units as there are number of treatments, each treatment appearing precisely once in each block. The treatments are randomly allocated to the experimental units within each block and replicated in several blocks with a separate randomization for each block.

*Example*:

Four treatments *A*, *B*, *C* and *D* are assigned at random to the experimental units in each of three blocks.

| Block | | | | | |
|---|---|---|---|---|---|
| | 1 | *B* | *A* | *C* | *D* |
| | 2 | C | *B* | *D* | *A* |
| | 3 | B | *C* | *A* | *D* |

### 3.3 Latin Square Design

A design with two blocking factors where the $n$ levels (versions) of a factor are allocated to $n^2$ experimental units arranged in $n$ rows or $n$ columns in such a manner that each level (version) appears exactly once in each row and exactly once in each column. The rows and columns represent levels of two blocking factors.

*Example* ($n$ = 4)

| Factor 1 (rows) | Factor 2 (columns) | | | |
|---|---|---|---|---|
| 1 | *A* | *B* | *C* | *D* |
| 2 | *B* | *C* | *D* | *A* |
| 3 | *C* | *D* | *A* | *B* |
| 4 | *D* | *A* | *B* | *C* |

The treatments are shown by the Latin letters.

NOTE — Latin square designs are generally used to eliminate two block effects, not of primary interest in the experiment, by 'balancing out' their contributions. The blocks are customarily identified with the rows and columns of the square. For example, the rows might be days and the

columns operators. The number of versions ($n$) of the principal factor and of each of the block factors has to be the same. Randomization can be achieved by assigning at random the versions of the principal factor to the letters, randomly selecting a Latin square from the listings or by the procedures described in statistical tables and assigning the versions of the block factors at random to the rows and columns of the square [There are: 1 (2×2); 12 (3×3); 576 (4×4); 161280 (5×5) Latin squares. Of these, there are: 1(2×2); 1(3×3); 4(4×4); 56(5×5) 'standard' Latin squares in which the first row and first column are in alphabetical order, and from which the other Latin squares can be derived by permuting the rows and columns].

A basic assumption is that these block factors do not interact (cause differential effects) with the principal factor under study, or among themselves. If this assumption is not valid, the measure of residual error will be increased, and the effect of the factor is confounded with such interactions. The design is particularly useful, when the assumptions are valid, for minimizing the amount of the experimentation. Sometimes other principal factors are used in the block positions so that there may be three principal factors without any block factors. This is equivalent to a fractional factorial with the assumption of no interactions. Some fractional factorial designs form Latin squares and it may be more desirable to approach the problem from the fractional factorial viewpoint to understand the assumption being made concerning interactions.

### 3.4 Graeco-Latin Square Design

A design with three blocking factors where the $n$ levels (versions) of a factor are allocated to $n^2$ experimental units arranged in $n$ rows and $n$ columns in such a manner that each level (version) appears exactly once in each row and exactly once in each column and also appears with greek letter exactly once. The rows, columns and greek letters represent levels of three blocking factors.

*Example* ($n = 4$)

| Factor 1 (rows) | Factor 2 (columns) | | | |
|---|---|---|---|---|
| 1 | A $\alpha$ | B $\beta$ | C $\gamma$ | D $\delta$ |
| 2 | B $\delta$ | A $\gamma$ | D $\beta$ | C $\alpha$ |
| 3 | C $\beta$ | D $\alpha$ | A $\delta$ | B $\gamma$ |
| 4 | D $\gamma$ | C $\delta$ | B $\alpha$ | A $\beta$ |

Factor 3 — Latin letters
Factor 4 — Greek letters

NOTE — The comments in the note in 3.3 are also pertinent here, modified by the extension to four factors. Graeco-Latin squares are generally used to eliminate three block effects. A Graeco-latin square does not exist for squares of size 6.

A generalization of the Graeco-Latin square for more than four factors is known as the 'hyper Graeco-Latin square'.

### 3.5 Incomplete Block Design

A design in which the experimental units are grouped into blocks in which there are insufficient number of experimental units available within a block to run a complete set of treatments ('replicate') of the experiment.

### 3.6 Balanced Incomplete Block Design (BIBD)

An incomplete block design in which each block contains the same number $k$ of different versions from the $t$ versions of a single principal factor arranged so that every pair of versions occurs together in the same number $\lambda$ of blocks from the $b$ blocks.

*Example*:

$t = 7, k = 4, b = 7, \lambda = 2$

| Block | Versions of the principal factor | | | |
|---|---|---|---|---|
| 1 | 1 | 2 | 3 | 6 |
| 2 | 2 | 3 | 4 | 7 |
| 3 | 3 | 4 | 5 | 1 |
| 4 | 4 | 5 | 6 | 2 |
| 5 | 5 | 6 | 7 | 3 |
| 6 | 6 | 7 | 1 | 4 |
| 7 | 7 | 1 | 2 | 5 |

NOTE — The design implies that every version of the principal factor appears the same number of times $r$ in the experiment and that the following relations hold true:

$$bk = tr,\ r(k-1) = \lambda(t-1) \text{ and } b \geq t$$

For randomization, arrange the blocks and versions within each block independently at random. Since each letter in the above equations represents an integer, it is clear that only a restricted set of combinations ($t, k, b, r, \lambda$) is possible for constructing balanced incomplete block designs. However, given 5 integers ($t, k, b, r, \lambda$) satisfying the above 3 conditions, it is not necessary that a BIBD will exist.

### 3.7 Partially Balanced Incomplete Block Design (PBIBD)

An incomplete block design with $t$ treatments and $b$ blocks is called a PBIBD with $m$ ( $\geq 2$ ) associate classes if:

a) Each block contains $k$ ( $< t$) distinct treatments
b) Each treatment appears in $r$ blocks
c) There exists a relation among treatments satisfying, the following conditions:
   i) any two treatments are either 1st, 2nd, ..., $m^{th}$ associates, the relation being symmetric, that is, if a treatment $\beta$ is the $i^{th}$ associate of $\alpha$ then so is $\alpha$ to $\beta$;

ii) each treatment has $n_i$ $i^{th}$ associate, $i = 1, 2, \ldots, m$; the number $n_i$ being independent of the treatment chosen;

iii) given a pair of treatments $(\alpha, \beta)$ which are mutually $i^{th}$ associates, the number of treatments that are simultaneously $j^{th}$ associate of $\alpha$ and $k^{th}$ associate of $\beta$ is $p^i_{jk}$, i, j, k = 1, 2, ... $m$. The number $p^i_{jk}$ is independent of the pair $(\alpha, \beta)$ of $i^{th}$ associate.

d) Any two treatments that are mutually $i^{th}$ associates appear together in $\lambda_i$ blocks $(i = 1, 2, \ldots, m)$, not all $\lambda_i$'s are equal.

e) The integers, $t$, $b$, $r$, $k$, $\lambda_1, \lambda_2, \ldots, \lambda_m, n_1, n_2, \ldots, n_m$ and $p^i_{jk}$ $i, j, k = 1, 2, \ldots, m$ called the parameters of a PBIBD, are connected by the following relations:

$tr = bk$

$n_1 + n_2 + \ldots + n_m = t - 1$

$n_1 \lambda_1 + n_2 \lambda_2 + \ldots + n_m \lambda_m = r(k - 1)$

$\Sigma p^i_{jk} = n_j - \delta_{ij}$ where, $\delta_{ij}$ is the Kronecker delta taking the value 1 if $i = j$ and 0, otherwise $n_i p^i_{jk} = n_j p^j_{ik} = n_k p^k_{ij}$

NOTE — PBIBD is an incomplete block design in which each block contains the same number $k$ of different versions from the $t$ versions of the principal factor. They are arranged so that not all pairs of versions occur together in the same number of the $b$ blocks; some versions can therefore be compared with greater precision than others. The design implies that every version of the principal factor appears the same number of times $r$ in the experiment.

*Example*:

$t = 6, k = 4, b = 6, r = 4, n_1 = 1, n_2 = 4, \lambda_1 = 4, \lambda_2 = 2$

| Block | Versions of the principal factor | | | |
|---|---|---|---|---|
| 1 | 1 | 4 | 2 | 5 |
| 2 | 2 | 5 | 3 | 6 |
| 3 | 3 | 6 | 1 | 4 |
| 4 | 4 | 1 | 5 | 2 |
| 5 | 5 | 2 | 6 | 3 |
| 6 | 6 | 3 | 4 | 1 |

In this design every version occurs $r = 4$ times and if we start with any version (for example, version 1), we find $n_1 = 1$ version (for example, version 4) that appears together with version 1 in $\lambda_1 = 4$ blocks and $n_2 = 4$ versions (No. 2, 3, 5 and 6) that appears together with version 1 in $\lambda_2 = 2$ blocks. These parameters $n_1, n_2, \lambda_1$ and $\lambda_2$ are the same whatever the starting version may be.

### 3.8 Youden Square Design

A type of design with two block factors called as rows and columns with the number of levels of one of the blocking factors, say, columns, equals the number of treatments and the other block factor has levels smaller than the number of treatments. The treatments are allocated in such a manner that every treatment appears once in each row and the columns when treated as blocks form a BIBD.

*Example*:

4×7 Youden Square Design

| | | | | | | |
|---|---|---|---|---|---|---|
| 3 | 4 | 5 | 6 | 7 | 1 | 2 |
| 5 | 6 | 7 | 1 | 2 | 3 | 4 |
| 6 | 7 | 1 | 2 | 3 | 4 | 5 |
| 7 | 1 | 2 | 3 | 4 | 5 | 6 |

NOTE — It can be seen that rows form a Randomized block design and columns a BIBD with parameters, $t = 7 = b$, $r = k = 4$, $\lambda = 2$.

### 3.9 Split-Plot Design

A design in which the group of experimental units (plot) to which the same version of a principal factor is assigned is subdivided (split) so that one or more additional principal factors may be studied within each version of that factor.

*Example*:

Three versions of factor $A$ are tested in two replicate runs. Within each version of $A$, the same two versions of factor $B$ are studied.

| | | Replicate I | | Replicate II | |
|---|---|---|---|---|---|
| Plot | $A_1$ | $A_1B_2$ | $A_1B_1$ | $A_1B_2$ | $A_1B_1$ |
| | $A_2$ | $A_2B_1$ | $A_2B_2$ | $A_2B_1$ | $A_2B_2$ |
| | $A_3$ | $A_3B_1$ | $A_3B_2$ | $A_3B_2$ | $A_3B_1$ |

NOTE — In the example, replicates serve the role of blocks to the first-stage principal factor ($A$) and each plot assigned to one of the three versions of $A$ serves the role of blocks for the additional second-stage principal factor $B$ (within plot factor) studied within A. Thus, the experimental error for the within-plot factor $B$ should be smaller than that for the full experiment (if there indeed is some effect of varying the first factor). In a split-plot design, different measures of residual error are obtained for the within-plot and the between-plot effects. It is possible to extend this design further in order to introduce a third-stage factor. This type of design is frequently used where large runs or areas are obtainable from a factor the levels of which are not easily changed, and the other factors can be varied readily within the runs or areas.

This type of arrangement is common in industrial experimentation as well as in agriculture (whence the name is derived). Frequently, one series of treatments requires a larger experimental unit while another series can be compared with smaller amounts. For instance, the comparison of different types of furnaces used to prepare an alloy would need greater amounts of alloy than the comparison of different types of moulds into which the alloy might be poured. The types of

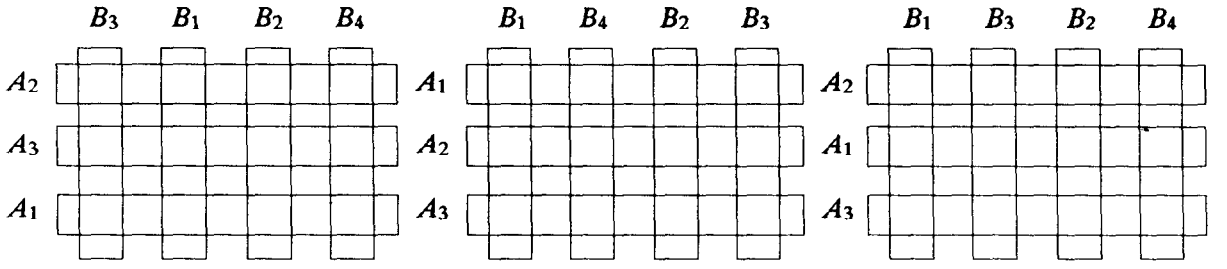
furnaces are regarded as the versions of the first-stage factor and the types of moulds as the versions of the second stage (within-plot) factor. Another example is a large machine the speed of which can be changed only by replacing the gear train, a time-consuming and expensive task, so that infrequent changes to this factor are desired. The material manufactured at each speed can be heat-treated by several techniques, shaped under varying pressures and smoothed using different polishing agents with relative ease of shifting from one level (version) of these factors to another. These latter constitute the within-plot factors (or second-stage factors) while the speed variations constitute the between-plot factor (or first-stage factor).

### 3.10 Split-Block Design; Two Way Split-Plot Design

A split-plot design in which the versions of the second-stage factor, instead of being randomized independently within each plot, are arranged in strips across plots in each replication. Thus, it is considered as a split-plot design in two different ways.

*Example*:

For a 3×4 design, the appropriate arrangements (after randomization) might be as shown below:




NOTE — The design sacrifices precision on the main effects (average effects) of $A$ and $B$ in order to provide higher precision on the interactions (differential effects), which will generally be more accurately determined than in either randomized blocks or the ordinary split-plot design.

In industrial experimentation, practical considerations sometimes necessitate its use; for example, in the textile industry, factor $A$ may be different procedures of bleaching by chlorine peroxide and factor $B$ those of rising by different amounts of hydrogen peroxide in the cooling process.

### 3.11 Mixture Experiment

An experiment in which two or more ingredients or components shall be mixed and the response is a property of the resulting mixture that does not depend upon the amount of the mixture. The proportions of each of the $q$ components ($X_i$) in the mixture shall satisfy the conditions $0 \leq X_i \leq 1$ and $\sum_{i=1}^{q} X_i = 1$; and each experimental point is defined in terms of these proportions.

NOTES

1 In some fields of application the experiment mixtures are described by the terms 'formulation' or 'blend'. The use of mixture designs is appropriate for experimenting with the formulations of manufactured products, such as paints, gasoline, foods, rubber and textiles.

2 In some applications, the proportions of the components of the mixture may vary between 0 and 100 per cent of the mixture ('complete domain'). In others, there may be operative restraints, so that at least one component cannot attain 0 or 100 per cent ('reduced domain').

### 3.12 Factorial Experiment (general)

An experiment in which all possible treatments formed with two or more factors, each being studied at two or more levels (versions), are examined so that interactions (different effects) as well as main effects can be estimated.

NOTE — The term is descriptive of obtaining the various factors in all possible combinations, but in itself does not describe the experiment design in which these combinations, or a subset of these combinations, will be studied.

The most commonly used designs for the selected arrangement of the factorial treatment combinations are the completely randomized design, the randomized block design and the balanced incomplete block design, but others also are used.

A factorial experiment is usually described symbolically as the product of the number of levels (versions) of each factor. For example, an experiment based on 3 levels of factor $A$, 2 levels of factor $B$ and 4 levels of factor $C$ would be referred to as a 3×2×4 factorial experiment. The product of these numbers indicates the number of factorial treatments.

When a factorial experiment includes factors all having the same number of levels, the description is usually given in terms of the number of levels raised to the power equal to the number of factors, $n$. Thus an experiment with three factors all run at two levels would be referred to as a $2^3$ factorial

experiment ($n$ being equal to 3) and has 8 factorial treatments.

Some commonly used notations for describing the treatments for a factorial experiment are:

a) Use a letter to indicate the factor and a numerical subscript the level (version) of the factor, for example, three factors $A$, $B$ and $C$ in a $2\times3\times2$ factorial experiment. The 12 combinations would be:

$A_1B_1C_1$, $A_2B_1C_1$, $A_1B_2C_1$, $A_2B_2C_1$, $A_1B_3C_1$, $A_2B_3C_1$, $A_1B_1C_2$, $A_2B_1C_2$, $A_1B_2C_2$, $A_2B_2C_2$, $A_1B_3C_2$, $A_2B_3C_2$

Sometimes only the subscripts, listed in the same order as the factors, are used, such as:

111, 211, 121, 221, 131, 231, 112, 212, 122, 222, 132, 232

Alternatively, one can denote the first, second and the third level by 0, 1 and 2 respectively to obtain the following combinations:

000, 100, 010, 110, 020, 120, 001, 101, 011, 111, 021, 121

b) Describe the levels in terms of the number of unit deviations from the centre level, including sign. In the case of an even number of levels where there is no actual treatment at the centre level, the coefficients describing the levels are usually given in terms of half-unit deviations. For example, with two levels, if a unit of deviation between these levels is 4 mm, the $-1$ coefficient might be assigned to 3 mm and the $+1$ to 7 mm with 0 being assigned to the non-included 5 mm level. In the above example the code would appear as

| | |
|---|---|
| $(-1, -1, -1)$; | $(+1, -1, -1)$; |
| $(-1, 0, -1)$; | $(+1, 0, -1)$; |
| $(-1, +1, -1)$; | $(+1, +1, -1)$; |
| $(-1, -1, +1)$; | $(+1, -1, +1)$; |
| $(-1, 0, +1)$; | $(+1, 0, +1)$; |
| $(-1, +1, +1)$; | $(+1, +1, +1)$; |

This descriptive coding has many advantages, particularly in analysing contrasts when levels are equally spaced. Unequal spacing of the levels or weighted emphasis for the various levels can also be reflected in the coefficients.

### 3.13 $2^n$ Factorial Experiment

A factorial experiment in which n factors are studied, each at two levels (versions).

NOTE — The $2^n$ factorial experiment is a special case of the general factorial experiment (*see* 3.12). A popular code for representing treatments in a $2^n$ factorial experiment is to indicate a factor at a high level by the corresponding small letter and the letter is omitted when the factor is at a low level. When all the factors are at low level the treatment code is (1).

*Example illustrating the Note above*:

A $2^3$ factorial experiment with factors $A$, $B$ and $C$:

| | Level | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Factor $A$ | Low | High | Low | High | Low | High | Low | High |
| Factor $B$ | Low | Low | High | High | Low | Low | High | High |
| Factor C | Low | Low | Low | Low | High | High | High | High |
| Code | (1) | a | b | ab | c | ac | bc | abc |

This type of identification has advantages for defining blocks, confounding and forming aliases.

Factorial experiments, regardless of the form of analysis used essentially involve forming contrasts of various treatments.

*Example illustrating contrasts*:

In a $2^n$ factorial experiment with factors $A$ and $B$

$A$: $[a - (1)] + [ab - b]$

is the contrast for studying the effect of factor $A$ at the 'low' level of $B$ plus the contrast for studying the effect of factor $A$ at the 'high' level of $B$.

$B$: $[b - (1)] + [ab - a]$

is the contrast for studying the effect of factor $B$ at the 'low' level of $A$ plus the contrast for studying the effect of factor $B$ at the 'high' level of $A$.

$AB$: $[ab - b] - [a - (1)] = [ab - a] - [b - (1)]$

is the contrast of the contrasts for studying the effects of factor $A$ at the 'high' level of $B$ and at the low level of $B$ or the contrast of the contrasts for studying the effects of factor $B$ at the 'high' level of $A$ and at the 'low' level of $A$.

Each contrast can be derived from 'symbolic product' of two terms, these terms being of the form $(a \pm 1)$, $(b \pm 1)$, where $-1$ is used when the capital

letter ($A, B$) is included in the contrast and +1 when it is not.

Thus,

$A$ : $(a-1)$ $(b+1)$
$B$ : $(a+1)$ $(b-1)$
$AB$ : $(a-1)$ $(b-1)$

These expressions are usually written in a standard order. In this case,

$A$ : $-(1)+a-b+ab$
$B$ : $-(1)-a+b+ab$
$AB$ : $(1)-a-b+ab$

Note that the coefficient of each treatment combination in $AB$ (+1 or −1) is the product of the corresponding coefficients in $A$ and $B$. This property is general in $2^n$ factorial experiments. After normalization, the $A$ term represents the effect of $A$ averaged over the two levels of $B$, that is, a 'main effect' or 'average effect'. Similarly $B$ represents the average effect of $B$ over both levels of $A$. The $AB$ term contrasts the effect of $A$ at the high and the low levels of $B$ (or the effect of $B$ at the high and low levels of $A$), that is, an 'interaction' or 'differential effect'.

### 3.14 Completely Randomized Factorial Design

A factorial experiment (including all replications) run in a completely randomized design.

### 3.15 Randomized Block Factorial Design

A factorial experiment run in a randomized block design in which each block includes a complete set of factorial treatments.

### 3.16 Confounded Factorial Design

A factorial experiment in which only a fraction of the treatments are run in each block and where the selection of the treatments assigned to each block is arranged so that one (or more) prescribed effect(s) is (are) confounded with the block effect(s), while the other effects remain free from confounding. All factor level combinations are included in the experiment.

*Example*:

In a $2^3$ factorial experiment with only room for 4 treatments per block, the $ABC$ interaction [$ABC : -(1)+a+b-ab+c-ac-bc+abc$] can be sacrificed through confounding with blocks without loss of any other effect if the blocks include:

| *Block* 1 | *Block* 2 |
|---|---|
| (1) | *a* |
| *ab* | *b* |
| *ac* | *c* |
| *bc* | *abc* |

NOTE — The treatments to be assigned to each block can be determined once the effect(s) to be confounded is (are) defined. Where only one effect is to be confounded with blocks, as in this example, the treatments with a positive sign are assigned to one block and those with a negative sign to the other. There are generalized rules for more complex situations. A check on all the other effects ($A, B, AB$, etc) will show the balance of the plus and minus signs in each block, thus eliminating any confounding with blocks for them.

### 3.17 Factorial Experiment with Partial Confounding

A factorial experiment with several replicates in at least one of which some main effects or inter- actions, confounded in other replicates are free from confounding.

*Example*:

In *a* $2^3$ factorial experiment requiring the use of blocks of 4 (*see* **3.16**) and carried out with 2 replicates, the following arrangement is selected so that the $ABC$ interaction is confounded in replicate 1 and the $BC$ interaction in replicate 2:

| *Replicate* 1 | | *Replicate* 2 | |
|---|---|---|---|
| Block 1 | Block 2 | Block 1 | Block 2 |
| (1) | a | (1) | *b* |
| *ab* | *b* | *a* | *c* |
| *ac* | *c* | *bc* | *ab* |
| *bc* | *abc* | *abc* | *ac* |

The estimate of $BC$ can be obtained only from replicate 1 and that of $ABC$ only from replicate 2. The remaining estimates of $A$, $B$, $C$, $AB$ and $AC$ are obtainable using both replicates and therefore will have greater precision.

### 3.18 Factorial Experiments with Total Confounding

A factorial experiment with several replicates in which some interactions or main effects are confounded in all replicates.

### 3.19 Fractional Factorial Design

A factorial experiment with only an adequately chosen fraction of the treatments required for the complete factorial experiment. This procedure is sometimes called 'fractional replication'.

NOTE — The fraction selected is obtained by choosing one or several 'defining contrasts' which are considered of minor importance, or negligible, generally interaction(s) of high order. These 'defining contrasts' cannot be estimated and thus are sacrificed. By 'adequately chosen' is meant selection according to specified rules which include consideration of effects to be confounded and aliased.

Fractional factorial designs are often used very effectively in screening tests to determine which factor or factors are effective, or as part of a sequen-

tial series of tests, but there are risks of getting biased estimates of main effects or of misjudging the relative importance of various factors. When there is a large number of factor level combinations resulting from a large number of factors to be tested, it is often impracticable to test all the combinations with one experiment. In such cases resort may be made to a fractional, that is partial replication. The usefulness of these designs stems from the fact that, in general, higher order interactions are not likely to occur. When this assumption is not valid, biased estimates will result.

*Example*:

Two half-replicates of a $2^4$ factorial experiment (*see* 3.13) for the code interpretation with defining contrast : *ABCD*

| + | − |
|---|---|
| *abcd* | *abc* |
| *ab* | *abd* |
| *ac* | *acd* |
| *ad* | *bcd* |
| *bc* | *a* |
| *bd* | *b* |
| *cd* | *c* |
| (1) | *d* |

Either of these half-replicates can be used as a 'fractional replicate'.

NOTE — In the example, the factorial combinations in the first column are those with the a '+' (plus) sign in the development of the symbolic product of the *ABCD* 'defining contrast' as illustrated in the example of 3.13.

$$ABCD : (a-1)(b-1)(c-1)(d-1)$$

Those factorial combinations in the second column are those with a − (minus) sign.

Because only those elements of the *ABCD* interaction having the same sign are run, (no *ABCD* contrast measure is obtainable) so that the *ABCD* interaction is completely confounded and inestimable. In addition, it will be found that because only half of the full factorial experiments is run, each contrast represents two effects.

From the + sign fractional replicate in the above example, we would compute the factorial effects as follows:

$$A : abcd + ab + ac + ad - bc - bd - cd - (1) : BCD$$

$$AB : abcd + ab + cd + (1) - ac - ad - bc - bd : CD$$

Effects represented by the same contrast are named 'aliases'. Note that, had the complete set of factorial treatments been run instead of only half of them, the estimates of the *A* and *BCD* or *AB* and *BC* effects would no longer be identical. That is

$$A : (a-1)(b+1)(c+1)(d+1)$$

is not equal to

$$BCD : (a+1)(b-1)(c-1)(d-1)$$

when all 16 combinations are included instead of only 8. This example, and the comments thereon, have been limited to the $2^n$ factorial experiments. A comparable, but more difficult, approach is available when there are more than two versions, but another approach to these situations is through the use of the composite design defined in 3.21.

### 3.20 Orthogonal Design

A design in which all the effects can be estimated independently of one another.

A necessary and sufficient condition for a design to be an orthogonal design is that each level of one factor occurs with each level of the other factor with proportional frequency.

Mathematically, this condition may be rewritten as follows:

$$n_{ij} = \frac{n_{ij\,.} \times n_j}{N}$$

for every combination of $(i, j)$ level and every pair of columns.
where

$n_{ij}$ = number of times the level combination $(i, j)$ occurs in any two columns,

$n_i$ = number of times the level $i$ occurs in one column,

$n_j$ = number of times the level $j$ occurs in the other column, and

$N$ = Total number of experimental units.

*Example*

Table of orthogonal design derived for 1/2 replicate of a $2^4$ factorial experiment is shown below:

| Treatment No. | Array No. | | | | | | | Treatments for | |
|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | full $2^3$ factorial experiment | 1/2 replicate of $2^4$ factorial experiment |
| 1 | − 1 | − 1 | 1 | − 1 | 1 | 1 | − 1 | (1) | (1) |
| 2 | 1 | − 1 | − 1 | − 1 | − 1 | 1 | 1 | *a* | *ad* |
| 3 | − 1 | 1 | − 1 | − 1 | 1 | − 1 | 1 | *b* | *bd* |
| 4 | 1 | 1 | 1 | − 1 | − 1 | − 1 | − 1 | *ab* | *ab* |
| 5 | − 1 | − 1 | 1 | 1 | − 1 | − 1 | 1 | *c* | *cd* |
| 6 | 1 | − 1 | − 1 | 1 | 1 | − 1 | − 1 | *ac* | *ac* |
| 7 | − 1 | 1 | − 1 | 1 | − 1 | 1 | − 1 | *bc* | *bc* |
| 8 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | *abc* | *abcd* |
| $2^3$ Factorial contrast name | *A* | *B* | *AB* | *C* | *AC* | *BC* | *ABC* | | |
| 1/2 replicate $2^4$ (or $2^{4-1}$) fractional factorial *contrast name* | *A* + *BCD* | *B* + *ACD* | *AB* + *CD* | *C* + *ABD* | *AC* + *BD* | *BC* + *AD* | *D* + *ABC* | (Aliased contrasts) NOTE — Defining contrast *ABCD* cannot be estimated. | |

NOTE — Orthogonal designs include a wide variety of special designs such as a latin square design (**3.3**), a completely randomized factorial design (**3.14**), a fractional factorial design (**3.19**) and so forth, which are already defined or derived. It is also possible, and useful, to construct an orthogonal design by using appropriate tables of orthogonal arrays in which the sum of products of elements in any pair of arrays, adjusted by the mean of the array, is equal to zero.

Statistical analysis of the results from experiments using orthogonal designs is generally relatively simple since each main effect and interaction may be evaluated independently. However, non-orthogonal designs, which may be planned or accidental (such as by the loss of data due to missing tests or gross errors), lead to more difficult, or sometimes impossible, statistical interpretation. The degree of difficulty is dependent on the nature of the non-orthogonality. *See* the first note in **4.6**.

### 3.21 Composite Design

A design developed specifically for fitting second order response surfaces to study curvature, constructed by adding further selected treatments to those obtained from a $2^n$ factorial (or its fraction).

*Example*:

If the coded levels of each factor are −1 and +1 in the $2^n$ factorial experiment [*see* notation (b) under comment on **3.12** factorial experiment (general)] the $(2n + 1)$ additional combinations for a 'central composite design' are:

$(0, 0, \ldots, 0)$, $(\pm\alpha, 0, 0 \ldots, 0)$,
$(0, \pm\alpha, 0, \ldots, 0)$, $(0, 0, \ldots, \pm\alpha)$.

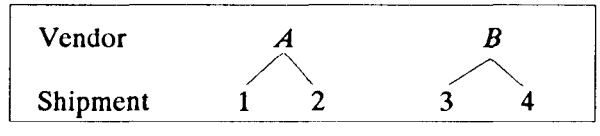
The total number of treatments to be tested is $(2^n + 2n + 1)$ for a $2^n$ factorial experiment. If $2^{n-k}$ fraction is taken, the number of treatments to be tested is $(2^{n-k} + 2n + 1)$.

NOTE — For $n$ = 2, 3 and 4 the experiment requires, 9, 15 and 25 units respectively, although additional duplicate runs of the centre point are usual, as compared with 9, 27 and 81 in the $3^n$ factorial experiment. The reduction in experiment size results in confounding, and thereby sacrificing, all information about interactions.

### 3.22 Nested Experiment; Hierarchical Experiment

An experiment to examine the effect of two or more factors in which the same level (version) of a factor cannot be used with all levels (versions) of the other factors.

*Example*:




If two vendors are to be compared by evaluating two shipments from each, there ordinarily is no direct relationship between the first shipment of vendor *A* and that of vendor *B* or similarly for the second shipment.

The differences between the two versions of the shipment factor of vendor *A* are nested within that

version of the vendor factor and, similarly, the differences between the two versions of the shipment factor of vendor *B* are nested within this other version of the vendor factor.

NOTE — Generally, nested experiments are used to evaluate studies in terms of components of variance rather than in terms of differences in response levels or prediction models. *See* the note in 4.4.

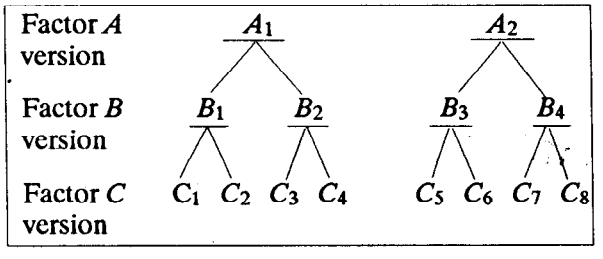
It is sometimes possible to redefine the factor into versions that can be compared across other factors if that makes a more meaningful question. For example, shipments 1 and 3 of the above example might represent Monday morning production and shipments 2 and 4 Friday afternoon production. The question could be framed in terms of Monday morning *versus* Friday afternoon production, which has a common thread, rather than in terms of two unrelated shipments. This would now represent a crossed [that is each level (version) of a factor is used with all levels (versions) of the other factors], rather than nested, classification and could be arranged as a factorial experiment.

| | Vendor | *A* | *B* |
|---|---|---|---|
| Day | Monday | 1 | 1 |
| | Friday | 2 | 2 |

### 3.23 Fully Nested Experiment

A nested experiment in which the second factor is nested within each level (version) of the first factor and each succeeding factor is nested within each level of the previous factor.

*Example*:




### 3.24 Staggered Nested Experiment

A nested experiment in which the nested factors are run within only a subset of the versions of the first or succeeding factors.

*Example*:

In the example for a fully nested experiment (3.23), version $C_3$ or $C_4$ and $C_7$ or $C_8$ might be eliminated, so that factor *C* is studied in only versions 1 and 3 of factor *B*. In this arrangement, the variability of *C* would be estimated with only half the precision of the arrangement in 3.23.

### 3.25 Partially Nested Experiment

A nested experiment in which several factors may be crossed as in factorial experiments and other factors nested within the crossed combinations.

NOTE — It is not unusual to find that experiments consist of both factorial and nested segments. *See* nested experiment (3.22).

## 4 METHODS OF ANALYSIS

### 4.1 Method of Least Squares

A technique of estimation of a parameter which minimizes $\Sigma e^2$, where $e$ is the random error component from the model.

NOTE — The experimental errors associated with the individual observations ordinarily are assumed to be independent, although the method may be generalized to the case of correlated errors. The usual analysis of variance, regression analysis and contrast analysis are all based on the method of least squares and provide different computational and interpretative advantages stemming from certain balances within the experimental arrangements which permit convenient groupings of the data.

### 4.2 Analysis of Variance (ANOVA)

A technique which subdivides the total variation of a set of data into meaningful component parts associated with specific sources of variation for the purpose of testing some hypothesis on the parameters of the model or estimating variance components.

An analysis of variance table usually contains columns for

- source of variation (first column);
- degrees of freedom (analogous to the denominator $n$-1 in the definition of sample variance $s^2$ ) (D.F.) (second column);
- sum of squares (S.S.) (third column);
- mean square (the sum of squares divided by the degrees of freedom) (M.S.) (fourth column).

Another column 'expected mean square E(M.S.)' may often be added to serve as a guide showing which mean squares under the model are to be compared in an *F*-test. When the levels (versions) are selected at random, the expected mean squares show the composition of the 'components of variance' assignable to the appropriate sources. *See* random effects model (4.4).

*Example*:

In a randomized block design the observation obtained from the $i^{th}$ of $t$ treatments in the $j^{th}$ of r blocks is denoted by $Y_{ij}$ ($i = 1, 2, \ldots, t$; $j = 1, 2, \ldots, r$). Then the following ANOVA table is computed:

**Analysis of Variance (ANOVA) Table**

| Source | Degrees of Freedom (D.F.) | Sum of Squares (S.S.) | Mean Square (M.S) | F | Expected Mean Square E(M.S.) |
|---|---|---|---|---|---|
| Block | $\nu_B = r - 1$ | $S_B = t \sum_j (Y_{ij} - Y_{.j})^2$ | $MS_B = \frac{s_B}{\nu_B}$ | $F(\nu_B, \nu_e) = \frac{MS_B}{MS_e}$ | $\sigma^2 + tK_B^2$ |
| Treatment | $\nu_t = t - 1$ | $S_t = r \sum_i (Y_{ij} - Y_{i.})^2$ | $MS_t = \frac{s_t}{\nu_t}$ | $F(\nu_t, \nu_e) = \frac{MS_t}{MS_e}$ | $\sigma^2 + rK_t^2$ |
| Error | $\nu_e = (t-1)(r-1)$ | $S_e = \sum_i \sum_j (Y_{ij} - Y_{i.} - Y_{.j} + Y_{..})^2$ | $MS_e = \frac{s_e}{\nu_e}$ | — | $\sigma^2$ |
| Total | $\nu_T = nt - 1$ | $S_T = \sum_i \sum_j (Y_{ij} - Y_{..})^2$ | — | — | — |

In the ANOVA table,

$S_T = S_t + S_B + S_e$

$V_T = V_t + V_B + V_e$

$F_{(\nu 1, \nu 2)}$ is the $F$-statistic.

The model associated with the observations is given as:

$$Y_{ij} = \mu + \alpha_i + \beta_t + e_{ij}; \quad i = 1, 2 \ldots, t; \quad j = 1, 2, \ldots, r$$

with

$$\Sigma \alpha_i = \Sigma \beta_j = 0 : e_{ij} \sim N(0, \sigma^2)$$

$$K_t^2 = \frac{\Sigma \alpha_i^2}{(t-1)}; \quad K_B^2 = \frac{\Sigma \beta_j^2}{(r-1)}$$

where

$\mu$ is the general mean;

$\alpha_i$ is the effect of the $i$th treatment;

$\beta_j$ is the effect of the $j$th block; and

$e_{ij}$ is the experimental error.

For this example, it is assumed that selected (fixed) levels are designated.

The least square estimates of $\mu, \alpha_i, \beta_j$ and $\sigma^2$ are obtained by:

$$\hat{\mu} = Y.. = \sum_i \sum_j Y_{ij}/rt$$

$$\hat{\alpha_i} = Y_{i.} - Y.. = \sum_j Y_{.j}/r$$

$$\hat{\beta_j} = Y.j - Y.. = \sum_i Y_{i.}/t$$

$$\hat{\sigma^2} = \sum_i \sum_j \frac{(Y_{ij} - Y_{i.} - Y.j + Y..)^2}{[(t-1)(r-1)]} = s_e^2$$

NOTE — Basic assumptions are that the effects due to all the sources of variation are additive and that the experimental errors are independently and normally distributed with zero mean and have equal variances (homoscedasticity) throughout all subdivisions of the data. The technique, in conjunction with the $F$ ratio, is used to provide a test of significance for the effects of these sources of variation and/or to obtain estimates of the variances attributable to these sources. The assumption of a normal distribution is required only for this test of significance and confidence intervals. Averages and interactions are usually looked at by summarizing in 2-way (or $k$-way) tables. This example assumes a *fixed effects model* (*see* 4.3). When the assumption of normal distributions of error cannot be made, it is sometimes possible to use transformations (for example logarithms, sine inverse, etc).

### 4.3 Fixed Effects Model

A model in which the levels (versions) of all factors are fixed rather than random selections over the range of versions to be studied for those factors.

NOTE — With fixed levels, it is inappropriate to compute components of variance.

### 4.4 Random Effects Model

A model in which the levels (versions) of all factors are assumed to be selected at random from the distribution of versions to be studied for those factors.

NOTE — With random levels, the primary interest is usually in obtaining components of variance estimates and it is inappropriate to compute estimates of the effects of the selected factor levels.

### 4.5 Mixed Effects Model

A model in which the levels (versions) of some factors are fixed, but for other factors they are selected at random.

NOTE — Components of variance are meaningful only for the random level factors and their interactions with 'fixed-effect' factors.

### 4.6 Regression Analysis

The process of estimating the parameters of a model by optimizing the value of an objective function (for example, by the method of least squares), and then testing the resulting predictions for statistical significance against an appropriate null hypothesis model.

NOTE — Regression analysis plays a role similar to the analysis of variance and is particularly pertinent when the levels of the factors are continuous and emphasis is more on the model than on the hypothesis tests. It is also useful for designated experiments with missing data since the balance required for ordinary use of the analysis of variance is not required for regression analysis. However, lack of balance increases the order-dependency (common elements are included in the first correlated term and not included in subsequent terms) of the hypothesis tests as well as losing other advantages of balanced experiments. For balanced experiments, the two techniques are simply variations of the method of least squares and produce comparable results.

*Example*:

A designed experiment which is orthogonally balanced, for example, three quantitative factors studied in a $2^3$ factorial, in which only a single replicate is run and the assumed model is selected as

$$Y = b_0x_0 + b_1x_1 + b_2x_2 + b_3x_3 + e$$

where

$x_0$ is equal to 1

$x_1$ is the coded level of factor $A$,

$x_2$ is the coded level of factor $B$,

$x_3$ is the coded level of factor $C$, and

$e$ is the random unexplained error.

**Regression Analysis Table for Example**

(*notation*: $x = X - \bar{X}$)

| Source | Regression Coefficient | Degrees of Freedom (D. F) | Sum of Squares (S.S.) | Mean Square (M.S.) |
|---|---|---|---|---|
| Regression of $Y$ on $X_1$ ($A$) | $B_1 = \frac{\Sigma x_{ij} Y_i}{\Sigma X_1^2 i}$ | 1 | $Sx_1 = B_1 \Sigma x_{1i} Y_i$ | $Sx_1$ |
| Regression of $Y$ on $X_2$ ($B$) | $B_2 = \frac{\Sigma x_{2i} Y_i}{\Sigma X_2^2 i}$ | 1 | $Sx_2 = B_2 \Sigma x_{2i} Y_i$ | $Sx_2$ |
| Regression of $Y$ on $X_3$ ($C$) | $B_3 = \frac{\Sigma x_{3i} Y_i}{\Sigma X_{3i}^2}$ | 1 | $Sx_3 = B_3 \Sigma x_{3i} Y_i$ | $Sx_3$ |
| Residual | — | 4 | $S_E$ : By Subtraction | $S_{E/4}$ |
| Total | — | 7 | $S_T = \Sigma Y^2$ | — |

NOTE — If the $2^3$ experiment were replicated within the same block the degrees of freedom for the 'total' would become 15 and the 'residual' would become 12. The 'residual' sum of squares might then be partitioned into 2 elements associated with 'replicates' and 'lack of fit' with 8 and 4 degrees of freedom respectively.

**Regression Analysis Table for Example**
**Addenda for Replicated Experiment**

| Source | Degrees of Freedom (D.F.) | Sum of Squares (S.S.) | Mean Square (M.S.) |
|---|---|---|---|
| Replicates | 8 | $S_R = \Sigma (Y_{ij} - Y_{i.})^2$ | $S_{R/8}$ |
| Residual | 12 | $S_E$ | $S_{E/12}$ |
| Lack of fit | 4 | $S_L = S_E - S_R$ | $S_{L/4}$ |

The statistical significance of each source is tested using the $F$-statistic for the mean square of that source and the appropriate error term. For the single replicate situation, the 'regression' terms would be tested against the 'residual' term. For the two replicates situation, the 'lack of fit' term would be tested against the 'replicates' ('experimental error') term to determine whether the model is inadequate, and the 'regression' terms would also be tested against 'replicates'. The 'replicates' term represents a measure of experimental error free of the potential contribution of model inadequacy which would be included in the 'residual' term.

### 4.7 Analysis of Covariance

A technique for estimating and testing the effects of treatments when one or more concomitant variables influence the response variable but they themselves are not influenced by the treatments.

NOTE — Usually the concomitant variable cannot be accounted for in the design of the experiment and its undesirable effect on the results has then to be taken into account in the analysis. For example, the experimental units may differ in the amount of some chemical constituent present in each unit, which can be measured, but not adjusted.

*Example*:

The model for a single factor analysis of variance ordinarily would be $\hat{Y}_{ij} = \mu_i$. Because of the concomitant variable $X_c$, an analysis of variance may produce biased results if not adjusted for. To account for the contribution of the concomitant variable, the model might become $\hat{Y}_{ij} = \mu_i + B(X_{cij} - X_{c..})$. A regression analysis could be used to obtain $B$ by pooling the within-version value for $\Sigma X^2_{cij}$ and $\Sigma X_{cij} Y_{ij}$ with $B = \frac{\Sigma (X_{cij} - X..) Y_{ij}}{\Sigma (X_{ij} - X..)^2}$. The $Y$ values could then be adjusted to account for the $X_c$ values.

## ANNEX A

*(Foreword)*

### DESIGN OF EXPERIMENTS

Design of experiments is essentially a strategy for experimentation that accounts for environmental conditions surrounding the experiments and for arranging the experiments so as to provide the answer to the questions of interest in an efficient, clear manner. Variability exists, and it must be taken into consideration. Studies of some factors under conditions of isolation where all other factors are held "constant" or at some "ideal" level, usually are not representative of what happens to that factor in the "real" world where there is simultaneous variation of many things.

Experimentation may take place in a laboratory where there is a high degree of freedom to change the levels of the factors of interest because the test specimens are not to be used after the experiment is over. In other cases, experimentation takes place in an existing process where there is a restriction to relatively small changes per step because the unit being studied (a person or a product) must be able to behave in a normal fashion following the experiment. The experiments may be run on "laboratory model" equipment requiring further work to relate to "production" status or they may be run in routine type environments.

While "design of experiments" (*see* 2.1) is independent in a sense from the analysis and interpretation of the data collected, frequently used analysis methods should be considered because they help in the understanding of design differences. The combination of design and methods of analysis (*see* 4) reflects how the design is effective.

In planning an experiment, it is necessary to limit biases introduced by the environment. For example, if those parts of the experiment using low dosage of a drug were conducted in the morning and those with high dosage in the afternoon, would the environmental factor of time of day be confounded with the levels of dosage? Topics such as "randomization" (*see* 2.11) and "blocking" deal with issues of how to minimize the unwanted effects of these "noise" elements that are usually so numerous they could not be eliminated even if it were economical or realistic to do so. Arrangements into "blocks" (*see* 3.2), "incomplete blocks" (*see* 3.5), "Latin squares" (*see* 3.3) and "split-plots" (*see* 3.9) provide mechanisms that let the experimenter consider beforehand how to reduce the effects of unwanted variability and how to get more meaningful answers.

The area of "factorial experimentation" (*see* 3.12) deals with the inter-relationships between multiple factors of interest to the experimenter. One-factor-at-a-time studies may be useful in some instances to gain insight into that factor, but they can also be misleading if that factor behaves differently

in the presence, absence or at other levels of other factors. Frequently the "breakthrough" that permits a step forward comes from the synergism revealed in a study of "interactions" (*see* 2.13), or a failure may stem from unknown interaction effects.

Factorial experiments may be at two versions or levels of each factor, which limits interpretation to linear relationships but may be sufficient for screening to determine if there is any apparent interest in the factor. They may also include three or more levels or versions to allow for estimation of "curvilinear" effects. The size of the experiment is an obvious consideration in experiment efficiency, and "fractional replication" (*see* 3.19), a means of selecting specific portion of a complete factorial experiment, is of immense value. For finding out which, if any, of the factors shows greatest promise of a real change, "screening" experiments using small fractional replications can be very effective. For work near the optimum points, curvature effects may be studied by the creation of "composite" designs (*see* 3.21)adding supplementary points to the two-level factorial experiment.

Experimentation is generally carried out to find factors of potential interest or to optimize some effects. For optimization, the data from the experiment is frequently used to create a "model" (*see* 2.21) of how the factors relate to selected levels. A "response surface" (*see* 2.22) serves as a map these models and may be useful in prediction and location of the next phase of experiments.

Good experiment design should:

a) furnish required information with minimum effort,

b) lead to pre-experiment determination of whether the questions of interest can be clearly answered in the experiment,

c) reflect whether an experiment series or a one-shot experiment is desirable,

d) show the pattern and arrangement of experiment points to avoid misunderstandings in carrying out the experiment and,

e) encourage the use of prior knowledge and experience in describing assumptions and selection of factors and levels.

## ANNEX B

*(Foreword)*

### LIST OF INDIAN STANDARDS ON STATISTICAL METHODS

| *IS No.* | *Title* |
|---|---|
| 397 (Part 1) : 1972 | Method for statistical quality control during production : Part 1 Control charts for variables |
| 397 (Part 2) : 1985 | Method for statistical quality control during production : Part 2 Control charts for attributes and count of defects |
| 397 (Part 3) : 1980 | Method for statistical quality control during production : Part 3 Special control charts |
| 397 (Part 4) : 1987 | Method for statistical quality control during production : Part 4 Master control systems |
| 1548 (Part 1) : 1981 | Manual on basic principles of lot sampling : Part 1 Itemized lot sampling |
| 2500 (Part 1) : 1992 /ISO 2859-1 : 1989 | Sampling inspection procedures: Part 1 Attribute sampling plans indexed by acceptable quality level (AQL) for lot-by-lot inspection |
| 2500 (Part 2) : 1965 | Sampling inspection procedures : Part 2 Inspection by variables for percent defective |
| 2500 (Part 3) : 1995 /ISO 2859-2 : 1985 | Sampling inspection procedures : Part 3 Attribute sampling plans indexed by limiting quality (LQ) for isolated lot inspection (Under Print) |
| 4905 : 1968 | Methods for random sampling |
| 5420 (Part 1) : 1969 | Guide on precision of test methods : Part 1 Principles and applications |
| 5420 (Part 2) : 1973 | Guide on precision of test methods : Part 2 Inter-laboratory testing |
| 6200 (Part 1) : 1995 | Statistical tests of significance : Part 1 $t$, Normal & $F$-tests |
| 6200 (Part 2) : 1977 | Statistical tests of significance : Part 2 $\chi^2$ Test |
| 6200 (Part 3) : 1984 | Statistical tests of significance : Part 3 Tests for normality |
| 6200 (Part 4) : 1983 | Statistical tests of significance : Part 4 Nonparametric tests |
| 7200 (Part 1) : 1989 | Presentation of statistical data : Part 1 Tabulation and summarization |

| *IS No.* | *Title* |
|---|---|
| 7200 (Part 2) : 1975 | Presentation of statistical data : Part 2 Diagrammatic representation of data |
| 7300 : 1995 | Methods for regression and correlation |
| 7600 : 1975 | Analysis of variance |
| 7920 (Part 1) : 1994 | Statistical vocabulary and symbols : Part 1 Probability and general statistical terms |
| 7920 (Part 2) : 1994 | Statistical vocabulary and symbols : Part 2 Statistical quality control |
| 7920 (Part 3) : 1995 | Statistical vocabulary and symbols : Part 3 Design of experiments |
| 8900 : 1978 | Criteria for the rejection of outlying observations |
| 9300 (Part 1) : 1979 | Statistical models for industrial applications : Part 1 Discrete models |
| 9300 (Part 2) : 1989 | Statistical models for industrial applications : Part 2 Continuous models |
| 10427 (Part 1) : 1982 | Designs for industrial experimentation : Part 1 Standard designs |
| 10427 (Part 2) : 1986 | Designs for industrial experimentation : Part 2 Orthogonal arrays |
| 10645 : 1983 | Method for estimation of process capability |
| 12347 : 1988 | Analysis of means — A graphical procedure |
| 12348 : 1988 | Use of probability papers |
| 13131 : 1991 | Statistical tolerance interval — Methods for determination |
| 14277 : 1995 | Statistical interpretation of test results — Estimation of mean, standard deviation and regression coefficient — Confidence interval (*under print*) |
| SP 28 : 1994 | Handbook on statistical quality control |

## ANNEX C

*( Foreword )*

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# ANNEX D

*( Foreword )*

## INDEX

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This Indian Standard has been developed from Doc : No. MSD 3 ( 31 ).

**Amendments Issued Since Publication**

| Amend No. | Date of Issue | Text Affected |
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